of another element which exists in the form of two isotopes differing much less in their atomic weights than those of chlorine. Furthermore, if we put $A = f(t)$, and if $f(t)$ can be expanded in the analytical series,

$$f(0) + t.f'(0) + \frac{t^2}{\underline{|2}}.f''(0) +, \text{ etc.},$$

and $f'(0)$ is zero as Nernst assumes, then it must be explained how the series

$$f(0) + \frac{t^2}{\underline{|2}}f''(0) +, \text{ etc.},$$

can become almost equal to $t.R \log_e 4$ between wide limits of temperature.

In fact, if the isotopes are inseparable by chemical means, I think that the most natural conclusion to draw is that the difference in the entropies of the reactants and resultants of a chemical change taking place at zero temperature is a finite quantity which depends on the type of the change, and also, of course, on the number of molecules transformed.

Jesus College, Oxford. D. L. CHAPMAN.

Anti-Gas Fans.

OWING to my absence from home I did not see Prof. Allmand's letter in NATURE of June 10 on my indictment of the War Office until too late to reply to it last week, but I hope you will now allow me to put before your readers a few of the points he has missed.

First, may I repeat that I had no personal interest in the number of fans sent out, since I neither asked for nor would accept payment or reward *of any kind or description* for their use during the war. I attributed the suffering and loss of life, which I deplore and Prof. Allmand denies, even more to the lack of training, and consequent ignorance of what the fans could do, than to their scarcity.

From this letter I gather that his own knowledge concerning them is of the slightest. He seems never to have heard of the clearing of trenches with them, the purpose to which they were principally put; but he allows that they were "found useful" for clearing shelters and dug-outs that would otherwise have remained dangerous for "hours, or even days," after a gas attack. We have only to picture our men, after hours of hard fighting, perhaps wounded or already gassed, compelled to remain in the open, whatever the weather, with the remains of gas still there, to realise the vital importance of clearing every space, dug-out, and shelter the moment it was possible. Yet for a whole year (May, 1915, to May, 1916) the use of the fans with which it could always have been done in a few minutes was held up by the wilful obstruction of War Office officials. It must be remembered, too, that at the beginning of that year the respirators were still quite crude and untrustworthy, and that even by the end of it they were very far from perfect. Is it too much to say, then, that "much suffering and loss of life could have been avoided" had the fans been accepted, and the troops properly trained to use them, nine months earlier, as they could so well have been? On this point Prof. Allmand is silent.

He says that, later, fires were found to be as efficacious as, and less fatiguing than, fans. They were not as efficacious, but they were certainly less fatiguing, as I have said, when the materials were ready to hand; and it was perfectly right to use them when practicable. But each time a space was cleared by fire, fresh *dry* wood and paper were required. Now it is common knowledge that there were wide areas which, over long periods, were so wet that dry wood for even one clearing would have been hard to find, let alone many. Is it an exaggeration to say that "much suffering and loss of life could have been avoided" if those responsible had remembered this, and had provided not only plenty of fans for every area, but also men trained to use them? On this point also Prof. Allmand is silent.

"Working an Ayrton fan, *even in the most approved fashion*"—my italics—he says, ". . . is a tiring task." Prof. Allmand will, I am sure, be surprised to learn that there are at least three approved fashions, and that the efficiency of the fans depends almost more on the ability of the officer in charge to choose the right method for the particular space, and to place and move his men properly, than on the skill of the wielders; moreover, the approved methods are not less fatiguing than the wrong ones, only infinitely more efficient. This ignorance on the part of an authority on anti-gas methods is not unique; it is typical. Let me show how it arises.

As soon as the fans were accepted I warned the War Office that, if they were to be of any value, officers and men alike *must* have two or three days' practical training in their use; and, at the request of the Commander-in-Chief, my assistant went to France to show how the training should be carried out. At first, after he left, it may have been fairly well done; but in time those who had seen for themselves what the fans could do died or became scattered; and after that the training degenerated at best into an hour or two of exercise in the stroke for trench-clearing, and at worst into the mere exhibition and naming of a fan, while numbers of men never even saw one at all. Major Gillespie, D.S.O., who practically saved his battery by means of the fans, when a howitzer battery within a hundred yards of it was wiped out, wrote to the *Times* of May 4: "Even after the introduction of the fans in limited quantities, I never met an officer or man who had been properly instructed in their use." This ignorance, for which the War Office is responsible, extended from the highest to the lowest officials in the Anti-Gas Service. The only men who did not share it were those officers who were clever enough and interested enough to make out for themselves what could be done with the fans. Small wonder, then, that most officers regarded them simply as nuisances, and that some of the men treated them as fuel.

This same ignorance and want of imagination led to the idea that the fans were useless for dealing with mustard gas. In describing the saving of his battery Major Gillespie wrote: "The gas hung about for days afterwards, but by judicious flapping at frequent intervals we kept our quarters fairly free from it." This is the evidence of a "fighting soldier." It is odd that those other fighting soldiers quoted by Prof. Allmand should not have thought of this very simple way of ridding themselves of a vapour that came off slowly and took some time to reach a dangerous concentration.

Finally, is not Prof. Allmand in his last sentence confusing scientific methods with the methods of some scientific men? In directing attention to the dire effects of the unscientific methods of the War Office in connection with anti-gas fans, I was adding my quota to the efforts of those who are trying to "ensure the application of scientific methods to military problems." The fact that it was scientific men who were responsible for those unscientific methods is surely no reason for condoning them, but rather for censuring them the more severely.

HERTHA AYRTON.

41 Norfolk Square, W.2, June 22.

AT the risk of the accusation of shirking inquiry, I repeat that I have no intention of entering into a

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controversy with Mrs. Ayrton. It will suffice to say that she is writing of things of which her knowledge is, naturally, second-hand, besides being clearly very inadequate. This is apparent in at least eight separate points in her letter, of which I will only refer to her mention of the successful use of the fans in what must obviously have been a *very* exceptional type of "mustard-gas" bombardment. I assure Mrs. Ayrton that she is mistaken if she imagines that she has in this matter any considerable body of support amongst those who knew the facts, from whatever point of view. I hope, in conclusion, that nothing I wrote has led Mrs. Ayrton to suppose that I regard her advocacy of her fans to be influenced by questions of "payment or reward." Nothing was further from my mind.

A. J. ALLMAND.

King's College, W.C.2, June 30.

PROF. ALLMAND, having read neither the specific charges I have made against the War Office nor the evidence, principally from official documents, with which I have sustained them, attempts to counter them with statements unsupported by evidence of any kind. He finds me ignorant, for instance, on eight points, of which the only one he names is obviously no matter either of my knowledge or ignorance, since it refers simply to a quotation from the letter of a very able and gallant "fighting soldier." Had he read the article he criticises he would have seen the whole quotation.

I am ready to sustain those charges, and to produce the evidence before any proper tribunal. I repeat them.

I accuse the War Office of having caused great loss of life and much avoidable suffering by:

(1) Having refused for a whole year to make use of anti-gas fans, which they were yet compelled finally to adopt owing to their proved efficacy.

(2) Never having set up an efficient organisation for training officers and men in their use, although I had warned them that this was indispensable.

(3) Having thus deprived the troops of the knowledge requisite for understanding what could be done with the fans, and having thereby induced the idea that they were useless.

(4) Having trusted entirely to fires for clearing dug-outs of gas, regardless of the fact that in many places dry wood and paper were often unobtainable.

(5) Ranking sandbags and ground-sheets as of equal efficacy with fans for clearing gas.

(6) Sending out an inadequate supply.

The scientific men implicated in these grave charges have not even made the plain statement with regard to them that the Editor of NATURE considered so desirable, much less produced any evidence in refutation of them.

HERTHA AYRTON.

July 11.

THE continuance of this correspondence in our columns would not, we think, serve any useful purpose. In a note in NATURE of May 13 it was pointed out that Mrs. Ayrton's indictment of the War Office was "not against the military element, but rather against the experts who were associated with the Gas Service." It is easy to understand the reluctance of these officers to express their views upon anti-gas fans, even if they were free to do so; and though Mrs. Ayrton is anxious to have all the facts judged by a tribunal appointed for that purpose, we must confess that the likelihood of a scientific body constituting such a tribunal is very remote. The inquiry is one that the Conjoint Board of Scientific Societies could take up appropriately, but no satisfactory conclusion could be reached without examining a number of witnesses, and the resources and powers of the Board are scarcely sufficient for such action. The only practicable course, therefore, would seem to be for the War Office to appoint a Committee to investigate Mrs. Ayrton's charges, and in the interests of scientific truth and efficiency we hope this will be done.—ED. NATURE.

The Stretching of Rubber in Free Balloons.

IN NATURE of June 10, p. 454, in connection with the attainment of high levels of the atmosphere by sounding- or pilot-balloons, Mr. W. H. Dines considers that such balloons would burst before reaching great heights, as the rubber of which these balloons are made would be stretched eightfold linearly, and he remarks that he does not think that any rubber will stand this treatment.

Properly vulcanised soft rubber will, however, stretch to more than ten times its original length if in the form of a ring-shaped test-piece. Moreover, the load increases more rapidly than the elongation at the later stages. The remarkable tensile properties of soft rubber are not always sufficiently recognised. The breaking strain of a properly vulcanised sample should be not less than 1500 grams per sq. mm. cross-sectional area of the original test-piece. Allowing for the stretching, which would reduce the cross-sectional area to one-tenth, the breaking strain would be 15,000 grams per sq. mm. cross-sectional area of the sample when fully elongated, or nearly 10 tons per sq. in. It would not, however, be safe to rely on these figures, as the rubber of the balloon would tear at the neck where it is tied together before the bursting pressure was reached. Mr. Dines has also failed to take into consideration the fact that part of the hydrogen would be lost by diffusion during the ascent of the balloon, which would reduce the pressure of the contained gas.

HENRY P. STEVENS.

15 Borough High Street, London
Bridge, S.E.1, June 29.

WITH reference to Mr. Stevens's interesting statements about the stretching of rubber, I think he has overlooked the fact that in a balloon the rubber is stretched simultaneously in both directions, whereas he refers apparently to one direction only.

I have cut a strip half an inch wide from a balloon used at Benson; it stretched sevenfold before breaking, but when extended sixfold its width was reduced from 0·50 in. to 0·22 in., instead of being extended to 3·00 in., as would be the case in actual use. Its unstretched thickness was 0·013 in., its thickness at breaking greater than 0·004 in., but when extended sixfold each way its thickness would only be 0·00036 in.

The loss of hydrogen by diffusion or leakage is equivalent to not giving the balloon so large a free lift at starting, and would alone increase the height, but in practice it sometimes leads to the bursting height not being reached at all because the free lift has vanished before that point is reached. It has been found that within fairly wide limits the maximum height is only slightly dependent on the free lift at starting. But diffusion of the hydrogen outwards is accompanied by diffusion of air inwards, and this increases the specific gravity of the gas and lessens the height.

I did not mention the effect of the tension of the rubber on the pressure, and therefore on the specific gravity, of the enclosed gas. Taking Mr. Stevens's figure of a breaking strain of 15,000 grams per sq. mm. of unstretched section, this will raise the internal pressure by quite an appreciable amount, and thereby reduce the height at which the balloon bursts.

Benson.

W. H. DINES.

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